

**Memorandum**

May 24, 1999

To: John Yearsley, EPA

From: Scott Wells, PSU, scott@eas.pdx.edu, (503)725-4276

Re: Peer Review comments on Temperature Modeling in Columbia River basin

Based on our phone conversation of 5/17/99 and your letter of 4/23/99 in response to my draft comments, I have enclosed a revised set of comments on the report entitled: "Columbia River Temperature Assessment: Simulation Methods."

Please contact me for further clarification on these comments. I have tried to be objective in evaluating the report you provided. Some of my comments may be based on only having partial understanding of the issues and modeling approach. Any further clarification would be useful for me. Thank you for the opportunity to work with you on evaluating this modeling effort.

## **Review of "Columbia River Temperature Assessment: Simulation Methods" by J. Yearsley, EPA**

Modeling the Columbia River is a difficult and complex undertaking. Dr. Yearsley is to be commended for approaching this task. Modeling such large systems has many challenges from modeling to data analysis. The review comments are divided into minor typographical and more substantive comments and critique. Two additional questions posed by Dr. Yearsley in a letter dated 4/23/99 were also evaluated.

### **Minor Typographical Comments**

1. p. 3 under "Hydrology" 3<sup>rd</sup> sentence change "can longer" to "can no longer"
2. p. 3 under "Water Resources Development" 2<sup>nd</sup> paragraph 2<sup>nd</sup> sentence change "The systems has" to "The systems have"
3. p. 5 2<sup>nd</sup> paragraph eliminate reference to "(Barnwell and Krenkel, 1982)" since it is redundant.
4. p. 7 1<sup>st</sup> paragraph change "(Cole and Buchak (1995))" to "(Cole and Buchak, 1995)"
5. p. 9 next to last paragraph, last sentence. Change "...such as dispersion and turbulent diffusion" to "...such as dispersion" since in a 1-D system turbulent dispersion is always >> turbulent diffusion.
6. p. 12 last sentence on page requires a period
7. Table 5: Data Source for 1<sup>st</sup> row reference is missing date
8. p. 20 1<sup>st</sup> paragraph last sentence change "Bonnevill" to "Bonneville"
9. p. 20 last paragraph change "xceedance" to "exceedance"
10. Figures 22-28: cannot tell from legend which graph is which
11. Figure 26: y-axis title is incorrect; it should be variance not temperature

### **Comments/Critique**

1. p. 4 Water Quality Issues

The 303(d) list for the State of Washington is used. Does this agree with the State of Oregon 303 (d) list for the Columbia River ? Perhaps a comment is required here to address State of Oregon 303 (d) issues, if there are any.

2. p. 7 Thermal Energy Budget

The assumptions in the development of Eq. 1 should be clearly stated as (1) no dispersion and (2) cross-sectional homogeneity (implying that all inflows mix laterally in the reach they are added).

- Is the assumption of no dispersion justified? Since the model uses steady-state hydraulics and computes only daily averaged temperature (as mentioned later in the report), dispersion may not be important. A calculation could show that

dispersion is not an issue. This can be done by computing the Peclet number, including the effect of heat transfer.

- Since the temperature standards are usually written in terms of daily maximum temperatures, the inclusion of dispersion in the temperature model will make a difference in the prediction of daily maximum temperatures or instantaneous temperatures. Dispersion therefore should be part of the model if the model were being used to evaluate instantaneous temperature standards in the Columbia River.

3. p. 8 Eq. 3 Define the term  $T_k$

4. p. 8 Eq. 4. Make sure that  $H_{\text{evap}}$  (also p.15 Eq. 19) is constrained always to be “evaporation” and not “condensation”. Using the formula of Eq. 19 one can obtain conditions where  $H_{\text{evap}}$  adds energy to the water body. This should never occur, and the model needs to be constrained that  $H_{\text{evap}}$  is only a loss of energy or zero.

5. p. 10 The choice of the mixed Eulerian-Lagrangian method is appropriate for this system. This reviewer does not see advantages to using this technique over other techniques since interpolating temperatures using a 2<sup>nd</sup> order polynomial introduces “diffusive” error into the Lagrangian technique that was trying to be avoided. Hence, there is no clear superiority of the Lagrangian technique over a purely Eulerian technique that has the same order of spatial accuracy.

6. p. 10 The “geometric properties of the river system” were assumed to be constant during a given time step. It was unclear whether this was the numerical time step or the “daily-averaged” temperature model period. If this assumption was for a daily time step, there could be large errors in the water balance if the time step were this coarse.

7. p. 12 Time and Length Scales

A discussion was made about the time and length scales of the forcing functions of the system. These can be determined explicitly from data sets (especially the 3-hourly meteorological data) using Spectral Analysis where the data are evaluated using Fourier transforms. Not only are the important frequencies determined from this type of analysis, but their relative importance is also determined.

8. p. 15 Heat Budget

The heat budget terms are properly formulated, but Eqs. 18-21 require the definition of several more terms before these can be evaluated, such as the terms  $\lambda$ ,  $E_v$ ,  $T_{\text{DB}}$ ,  $W$ ,  $e_s$ ,  $e_o$ ,  $e_a$ , etc.

9. p. 15 and 16 Initial Water Temperatures

A regression equation, Eq. 22, was used to compute stream temperatures based on air temperatures. With the 4 parameter model of Eq. 22, statistical curve fits probably are very good. A more accurate correlation though may be based on equilibrium temperatures rather than air temperatures. Using air or equilibrium temperatures may not affect the accuracy of the model. The comment that the stream temperatures predicted by Eq. 22 gave “good results even when the air temperature measurements were not in proximity to the stream gaging station locations” is an indication that the correlation is not a strong function of local air temperature but is only a result of calibration to a 4 parameter statistical model.

Also, the correlation is based on “weekly stream temperature” even though the in-stream model is “daily averaged” and the TMDL requirements for temperature are based on instantaneous daily maximums. The use of a weekly stream temperature was not explained in the report. Why were daily average temperatures not used in order to be compatible with the in-stream model temporal resolution?

#### 10. p. 17 Systems Model Bias and Error

The statement that “wind speed, cloud cover, relative humidity and station pressure are large-scale phenomena and that air temperature is a more local phenomenon” is not necessarily correct. I understand in the modeling of such a large system the modeler has to make approximations, but the above statement is not a good basis for ignoring local variability in wind, clouds, and humidity. An analysis showing the meteorological variability and the expected degree of error induced by neglecting that variability would be useful.

Using only 3 meteorological stations with high-resolution data does not seem to provide the required spatial reliability for such a large system. Echoing the comment above, a special effort should be undertaken to provide a much more rigorous analysis of meteorological data variability in the basin. Such information was not available for review in the report.

#### 11. p. 19 Uncertainty and Variability

The purpose for this modeling exercise was to “identify critical issues for additional study”. The peer review process is serving that purpose.

The statement was made that “the focus in this study was on the space-time complexity rather than on model complexity”. All modeling should be done at the appropriate level of complexity for the project – otherwise the usefulness of the model study is compromised. If the model used is not appropriate or does not provide enough detail to use for TMDL analyses, a more complex model should be used. This modeling objective and purpose may need to be included earlier in the paper.

Comments on each model component:

- Heat budget: components of the heat balance are reasonable but the model is adequate only for steady-state simulations since dispersion is neglected. There is some numerical "diffusion" because the Lagrangian solution is interpolated onto a grid. The model is not adequate for instantaneous temperature simulation.
- River hydraulics: model is adequate for steady-state hydraulics. Hence, for periods of unsteady-flows (snowmelt and storm conditions), the model is inadequate.
- Initial conditions – reasonable especially since they do not affect conditions long into the simulation
- Water Balance – Even though the conclusion is made that irrigation, groundwater return flow and miscellaneous tributary flows were only 5-7% of the flow increment in the Columbia, there is no reason not to include these if they are known since they probably have an important local effect.
- Filter – The discussion in the text was unclear as to how the Kalman Filter technique was used. Initially, this reviewer thought that the Kalman Filter was used in predicting the new state of the Columbia River without dams while using statistical estimates based on the existing system. In discussions with Dr. Yearsley, this was clarified. Apparently, the Filter was used only to estimate the variance of the estimate of temperatures from the deterministic model. In this sense, the Filter is appropriate to use. Apparently, figures 6-13 are the results of the deterministic model and not the results after using the Kalman filter. This should be clarified in the text.

Also, since many are familiar with Monte Carlo techniques, the text should at least mention why Kalman Filter techniques were chosen over Monte Carlo techniques for assessing the variance of the deterministic model estimate.

- The deterministic model as a whole (Heat balance equation including heat budget and river hydraulics): The deterministic model does not appear to be accurate during transition seasons. There needs to be statistics showing the mean error of the model at each location in figures 6-13. In general, a deterministic unsteady temperature model without any statistical filtering should have a mean error less than 1 and 2°C for instantaneous temperature predictions. If instantaneous temperatures are required to be predicted, a more complex model may be necessary.

12. In Figures 6-13: Temperature data in the winter approach 0°C even at Bonneville Dam in the winters of 1991 and 1993. Is this realistic?

13. In the graphs showing "Frequency of Exceedance" Figures 30-41, comparisons of graphs with and without dams and with tributary temperatures altered need to be on the same graph rather than on separate pages. This would aid the reader in evaluating comparisons between alternatives.

14. In the results section, much of the discussion relates to the average magnitude of exceedances of the management strategies. This is appropriate. But it should also be stated in the text that the temperature standard deviations of the alternatives many times overlap such that it is possible that there may be no difference between management strategies.

### **Evaluation of Model Objectives and Model Certainty**

In a letter of 4/23/99, Dr. Yearsely asked that 2 questions be evaluated. The questions and the evaluations are included below.

#### **1. Have the objectives of the temperature model been clearly identified?**

The model objectives were mentioned briefly on the bottom of p.1, end of p.4 and top of p. 5, and again on p. 18. It should probably also be stated in the report that this work is not meant to look specifically at the temperature TMDL issue. This becomes a little unclear when there is a discussion on the temperature TMDL on p.4 followed by the statement at the bottom of p. 4 that this is "a first step in developing a TMDL." If one were developing a model for the TMDL evaluation, the model chosen would be somewhat different as explained above in specific review comments.

#### **2. Has the level of certainty required by the model objectives been identified and can the proposed concept achieve this level of certainty?**

The level of certainty required to achieve the model objectives was not explicitly stated in the report and is a very subjective measure. This would be more easily evaluated if the model deterministic error were clearly shown relative to the expected differences predicted by the model for different alternatives. In general, though, the level of certainty of this work is consistent with the objectives of a screening model analysis.